sea-level. Furthermore, according to the theory of driven cyclones, the progressive motion of the cyclone is supposed to be determined by the "prevailing west component" of the upper currents. At Blue Hill the mean westerly component of the upper current is 35 metres per second at 9000 metres, about 17 metres per second at 4000 metres, and 1 metre per second at 200 metres (*Harvard Observatory Annals*, 1890, vol. xl. p. 447). It is natural to suppose that a driven whirl, in such conditions, would be rapidly toppled over and destroyed. Yet storms persist for days. If, however, a driven whirl did persist in such conditions, its axis if tilted at all would, according to all analogy, be tilted in the direction of progressive motion. Yet the direct observations with kites at Blue Hill, and the observations of clouds by Ley in England, prove that the axis of the cyclone is tilted backward. Moreover, it is reasonable to suppose that the air in the rear of a driven whirl would partake of the progressive motion of the whirl, and this, added to the indraught, would make the wind velocity in the rear of the whirl very much greater than that of the winds in front, yet such is not generally found to be the case. For these reasons I think the observations do not favour the theory of driven cyclones.

The theory of cyclones with which the observations in temperate latitudes seem best to agree, is the theory which supposes the cyclone to result from contrast of temperature in a horizontal direction. This I have called the *convection theory*. In this theory there are two types of cyclone. The warm-centre cyclone of the lower atmosphere, and the cold-centre cyclone of the upper atmosphere. The best type of the cold-centre cyclone is the polar cyclone; but there also exist in the upper air in temperate latitudes small travelling cyclones or *hemi*-cyclones of the same nature. Horizontal contrasts of temperature are most marked in winter, hence the theory explains why cyclones are most violent in winter. The origin of the horizontal contrasts of temperature is not shown by observation. They probably arise by the interchange of air between higher and lower latitudes. A body of air moving from the equator toward high latitudes would come into a region where it would be nearly surrounded by colder air, and the conditions would favour the production of a warm-centre cyclone. A body of air moving toward the equator would produce conditions favourable to a cold-centre cyclone. H. H. CLAYTON.

Blue Hill Meteorological Observatory, March 30.

Rock-structures in the Isle of Man and in South Tyrol.

IF the intercrossing of two separate systems of folding be the essential condition in the complicated rock-structures so ably worked out by Mrs. M. M. Ogilvie Gordon in South Tyrol, I scarcely think the parallelism with the conditions in the Manx *Carboniferous* rocks can be so close as Mrs. Gordon suggests in her recent letter (NATURE, March 22, p. 490).

So far as I have been able to judge, the disturbances in the Carboniferous volcanic rocks of the Isle of Man were the result of a movement which was single both as regards direction and time. It is true that this conclusion was reached in 1897, before Mrs. Gordon had taught us the importance of torsion-structure in areas of disturbance; but I re-examined the sections last autumn, after having studied Mrs. Gordon's paper, without finding any reason to alter my former opinion on this point. The interpretation given in my recent paper (*Q.J.G.S.* vol. lvi. p. 11) is therefore in all respects the same as that published in brief in the official Summary of Progress of the Geological Survey for 1897 (pp. 110-112).

It seems necessary, also, to call attention to the small scale of the structures in question in the Manx Carboniferous rocks. Their most striking feature is their sudden local development in a limited tract where the strata are rendered by diverse lithological composition peculiarly susceptible to differential displacement. Under such conditions, it appears that even a small degree of lateral movement may be so focussed as to cause great disturbance at certain places without much disturbance of adjacent tracts. The post-Carboniferous movement in the Isle of Man can scarcely have been even approximately of the magnitude of the disturbances in South Tyrol described by Mrs. Gordon.

It was in pre-Carboniferous times that the Manx region underwent earth-movements of really grand intensity; and Mrs. Gordon may have had this fact in mind in referring to the subject. In the Older Palæozoic (probably Cambrian) slate-rocks of the island, "crush-conglomerate" has been developed on a very extensive scale by differential shearing, as described by Prof. W. W. Watts and myself five years ago (*Q.J.G.S.* vol. li. p. 563). These rocks, moreover, show evidence of successive epochs of disturbance, varying slightly in direction but apparently all pre Carboniferous. The production of the "crush-conglomerate" appears to have occurred during only one of these stages. It is not improbable that an observer acquainted with the "torsion-structures" of the Dolomites might be able to find parallel phenomena among the highly complicated pressure-structures in the Manx Slates; but I think that a sharp distinction should be drawn between these structures and those of the Carboniferous rocks of the island. G. W. LAMPLUGH.

Tonbridge, April 8.

Electric Light Wires and Dust.

I BELIEVE that the collection of dust upon electric light wires and fittings is generally attributed to air currents, due to thermal causes, the same thing occurring, to some extent, with hot-water pipes. Recent experience has, however, convinced me that in the case of electric light conductors, electrostatic attraction is really the chief factor, particularly where the supply is at 200 volts from the street mains. In my office here I have several electric light cords strung across the ceiling. They are all exactly similar and under the same conditions, except that some of them have the switch in the negative and some in the positive conductor. The former gather dust to an extraordinary degree, and now, after a few months' use, have become quite an eyesore. The latter are practically as clean as when first put up. As is well known, the negative conductor of a street supply tends always to earth itself, and, as a matter of fact, in my case I find that the negative of my supply from the Westminster Co. is almost at earth potential. The positive, on the other hand, is nearly 200 volts above the potential of the earth. In this lies obviously the cause of the phenomenon. The wires which have the switch in the negative are nearly at 200 volts potential above the earth whenever the switch is off, while those which have the switch in the positive are at zero potential in these circumstances. Of course, when the switches are on, all the cords are under similar conditions, one conductor in each being nearly at 200 volts above earth, and the other at about earth potential. No doubt it is when the switch is off, in the case where it is in the negative conductor, that the accumulation of dust takes place. Having regard to the comparative lowness of the 200 volts potential, from an electrostatic point of view, the rate at which the dust accumulates on the cords is most surprising, and this is my reason for thinking it worth while bringing the matter to notice. A. A. C. SWINTON.

66 Victoria Street, Westminster, April 23.

ON THE SIZE AT WHICH HEAT MOVEMENTS ARE MANIFESTED IN MATTER.

IN the molecular theory of heat it is assumed that the motions of atoms and molecules are the motions upon which the phenomena of temperature depend. These motions are assumed to be very irregular, and the apparent uniformity of structure of a gas, for example, is attributed to the very small size and irregularity of the motions, which within any region of sensible size are the same, *on the average*, as within any neighbouring region. Within regions of molecular dimensions the distribution of motion is extremely irregular; neighbouring molecules are not in general moving at the same speed or possibly vibrating in the same way. Hence in this view the size at which heat movements are manifested in matter are of molecular sizes, *i.e.* from 10^{-7} to 10^{-8} cm.

In addition, however, to all this matter, motion and vibration, there are present ether motions of an irregular kind. Within any closed envelope at constant temperature the ether motions must be in statistical equilibrium with the motions in the envelope. The energy per c.c. of these ether motions will be considerable at high temperatures, and small at low ones. Many years ago I called attention to the energy per c.c. required in order to, in this sense, warm up ether, and showed that it was quite comparable with that required to warm up a rare